6107130016

March 21, 2014

Mr. James M. DiLorenzo
Remedial Project Manager
USEPA Region 1 - New England
5 Post Office Square
Mailcode: OSRR07-4
Boston, MA 02109-3912

9-640862

Subject: Response to USEPA's October 1, 2013 Comments Letter Concerning the July 26, 2013 "*Second Interim Deliverable – Baseline Ecological Risk Assessment*, *Operable Unit 1 and Operable Unit 2"* for the Olin Chemical Superfund Site, Wilmington, Massachusetts

On behalf of Olin Corporation (Olin), AMEC Environment & Infrastructure, Inc. (AMEC) respectfully submits the following responses United States Environmental Protection Agency's (USEPA's) October 1, 2013 comments prepared by Nobis Engineering, Inc. (Nobis) on the *"Second Interim Deliverable – Baseline Ecological Risk Assessment, Operable Unit 1 and Operable Unit 2"*, dated July 26, 2013 for the Olin Chemical Superfund Site in Wilmington, Massachusetts.

Below please find USEPA comments on the Interim Deliverable No. 2 followed by Olin's response. The comments and responses are arranged by section.

BASELINE ECOLOGICAL RISK ASSESSMENT GENERAL COMMENTS

Comment No. 1: The majority of the BERA comments provided by EPA for Interim Deliverable #1 were addressed. The only exception being comment #12 (see AMEC 26 July 2013 letter to James DiLorenzo, subject: Response to USEPA Comments, Draft Remedial Investigation Report for Operable Unit 1 and Operable Unit 2: Baseline Ecological Risk Assessment – Interim Deliverable No.1), no additional discussion of the source(s) of water or amphibian use was provided.

> ***Response No. 1***: The primary source of water in the Off-Property West Ditch (Off-PWD) is stormwater runoff from the Jewel Drive area. The installation of the weir resulted in suppression of groundwater and dramatic reduction/elimination of groundwater discharge to the Off-PWD. As noted in Table 3.5-1, frogs inhabit the Off-PWD. Frogs have been encountered in Off-PWD in the springtime (after winter thaw) until the Off-PWD dries out, often in early summer. Salamanders, salamander egg masses, or salamander larvae have not been observed in the Off-PWD.

Comment No. 2: The Baseline Exposure Assessment and Effects Estimate not only provided exposure assessment and toxicity information, but also present preliminary calculations of risk that are typically reserved for the Risk Characterization portion of the BERA

(i.e., risk results). It would have been a more efficient use of time to save the calculation of risk until after receiving comments on the exposure and effects approach, since numerous comments have been provided that directly impact risk results.

Response No. 2: No response required

Comment No. 3: This review focused on key components of the Exposure and Effects Estimate, but did not include a detailed review of most of the calculations provided. Given the nature and detail of the recommended changes it was decided to reserve detailed evaluation of risk calculations for the Interim Deliverable #3.

Response No. 3: No response required

Comment No. 4: It appears that COPECs are being selected based on no-effect screening benchmarks, consistent with a SLERA; however, it is unclear whether the benchmark comparisons in the BERA are based on no effect benchmarks or effect benchmarks. For instance, in Section 3.9, the measurement endpoints 1A, 2A, 3A, 4A, 7A, 8A are for "effects" benchmarks. Please clarify throughout the document whether benchmarks are no-effect benchmarks (e.g. Criterion Continuous Concentrations (chronic value), NOAEL-based EcoSSLs, NOAEL-based TRVs) or effect benchmarks (e.g. Criterion Maximum Concentrations (acute value), LOAEL-based EcoSSLs, LOAEL-based TRVs). While the SLERA should use conservative (e.g., NOAEL–based) values, where possible, the BERA should include LOAEL-based (less conservative) values so that a range of potential ecological risks can be identified. Included with these comments is an example of LOAEL-based soil benchmarks developed by the EPA Region 1 ESAT contractor (see Attachment 1).

Response No. 4: COPECs were selected using "no-effect" screening benchmarks based on NOECs/NOAELs. The effects assessment relies on "effects" benchmarks/TRVs based on LOECs/LOAELs so that a range of potential ecological risks can be identified. The BERA also incorporates LOAELs calculated from data presented in the USEPA Eco-SSL documents. The text will be revised to clarify when "effects" benchmarks are used.

SECTION 2.0 SPECIFIC COMMENTS:

Comment No. 5: Section 2.1. Page 2-1. Was MassDEP in agreement with the results presented in the 1997 Stage II ERC? If not, please provide information on any areas of disagreement.

Response No. 5: The 1997 Stage II ERC is approximately 16 years old, and a substantial amount of investigation has been performed at and around the site since then. Any of DEP's perspective on that historical MCP document are not necessary for, or relevant to, the preparation of the present CERCLA BERA program.

Comment No. 6: Section 2.2. Page 2-2. Did MassDEP agree with the conclusions Olin presents for the FRA? If not, please provide information on areas of disagreement.

Response No. 6: The 1997 Stage II ERC is approximately 16 years old, and a substantial amount of investigation has been performed at and around the site since then. Any of DEP's perspectives on that historical MCP document are not necessary for, or relevant to, the preparation of the present CERCLA BERA program.

Comment No. 7: Section 2.3. Page 2-3. This section seems out of place in a Section titled Previous Ecological Risk Assessments. This subsection might work better in Section 4.0 Baseline Exposure and Effects Estimate.

Response No. 7: The section 2.3 text concerning previous evaluations of ammonia in surface water has been retained and additional discussion of potential impacts of ammonia in surface water is included in the RI report itself and in the remainder of the BERA.

Comment No. 8: Section 2.3. Page 2-3. While the benchmarks previously used for ammonia may not be appropriate given existing Site conditions, there is no information presented to justify the claim that risks were overestimated as a result of its use. Please provide justification.

Response No. 8: Table 4 of the 2013 AWQC decision document for ammonia shows the ranked genus mean chronic values (GMCV) in terms of total available nitrogen (TAN) per liter. The most ammonia-sensitive species include species that are not native to Massachusetts (*e.g.* freshwater mussels indigenous to the Mid-West) or have never been observed in the waterbodies being evaluated (*e.g.* fish, freshwater mussels). The first non-fish/non-mussel taxon listed in Table 4 that could inhabit the ditch system is the amphipod. The corresponding GMCV is 29.17 mg TAN/L, a concentration nine times higher than the AWQC, thus using the AWQC over-estimates risk at the Site. The absence of fish is due to physical habitat restrictions (*e.g.* ephemeral hydrology) rather than ammonia toxicity. The absence of mussels is also therefore expected since they require fish hosts to disperse. The text will be revised to include this discussion.

SECTION 3.0 SPECIFIC COMMENTS:

Comment No. 9: Section 3.5.1. Page 3-5. *Thsuga* is correctly spelled *Tsuga*.

Response No. 9: Text will be revised as noted.

Comment No. 10: Section 3.5.1. Page 3-6. Olin should include wildlife likely (based on habitat conditions) to occur for each exposure area in Table 3.5-1, not just those species observed during weir inspection.

Response No. 10: The list of wildlife was compiled from many years of weekly inspections, and observations made during other field activities, and should be a sufficient indicator of which species are likely to inhabit the site over time. Also, USEPA 1997 Appendix B provides an example checklist for conducting a habitat assessment and identifying site-specific receptors. That check list does not include compiling a list of all wildlife likely to occur. The list was prepared consistent with available guidance. No revisions to the list are planned.

Comment No. 11: Section 3.5.9. Page 3-8. Olin had agreed to provide additional information on the source(s) of water for the Off-Property West Ditch (BERA comment #12). No such information has been added to this section.

Response No. 11: See response to General Comment No. 1.

Comment No. 12: Section 3.5.10. Page 3-8. Figure 1.0-2 is not provided in the BERA SID, Olin should consider changing this reference to Figure 3.2-2.

Response No. 12: The text will be revised to reference Figure 3.2-2.

Comment No. 13: Section 3.5.11. Page 3-9. A reference to Figure 3.2-2 would be helpful in this section.

Response No. 13: Text will be revised to reference Figure 3.2-2.

Comment No. 14: Section 3.9. Page 3-13. Table reference to Table 5.9-2 should be 3.9-2.

Response No. 14: Text will be revised as noted.

Comment No. 15: Section 3.9. Page 3-15. General. The 10/11 attributes from Menzie et al. (1997) were condensed to 5 attributes. In general, the approach is fine as there tends to be overlap between/among the attributes, particularly when there are not many measurement endpoints as at this Site. One attribute seemingly not accounted for was "Quantitative Measure" or "Quantitativeness." It would be helpful to the reader if for example, within the bullet list on 3-15, the Menzie et al. attribute terminology was included parenthetically to easily facilitate recognizing which attributes were combined in this approach.

Response No. 15: "Quantitativeness" is identified in Menzie *et al.* (1997) as a component of study design and execution. In the BERA, "study design and execution" is a component of the "data quality and study design" attribute. The Menzie *et al.* terminology will be added to the bullet list for clarification.

Comment No. 16: Section 3.11.1. Page 3-19. Please confirm that all South Ditch sediment samples used in the BERA are post remediation samples.

Response No. 16: Yes, South Ditch sediment samples used in the BERA were collected after the remediation. South Ditch was remediated in 2000. The samples used in the BERA date from 2005 to present.

Comment No. 17: Section 3.12.1. Page 3-22. Please provide the criteria used to select which study result (i.e., LC50 value) was selected from the ECOSAR database to be used to develop the chemical-specific benchmark. Was the lowest LC50 value used or were other criteria employed? Was any consideration given to matching the ECOSAR organism with conditions with the water body?

Response No. 17: ECOSAR predicts toxicity values for a substance based on molecular structure and empirical toxicity data for similar chemicals. ECOSAR provides only one value

for each available effect type; for example ECOSAR provides only one LC50 value. There is no opportunity to select the lowest LC50 value as only one will be presented. Aquatic organisms for which ECOSAR values are available are limited to three generic receptors: green algae, daphnids, and fish. Although fish have not been observed in the on property ditch system, the ECOSAR values for fish are generally the most conservative. Fish are suitable surrogates for amphibians, which do inhabit the site.

Comment No. 18: Section 3.13: there is additional aquatic and terrestrial toxicity information for hydrazine (CAS 302-01-2), technical 4-nonylphenol (CAS 84852-15-3), and Kempore (CAS 123-77-3) in the REACH database (http://echa.europa.eu/web/guest/information-onchemicals/registered-substances). These include Probable No Effect Concentrations (PNEC) in freshwater that are usable as no-effect benchmarks and toxicity data. In addition, there is a finch TRV for hydrazine in soil. Please incorporate these data as appropriate in the SLERA and BERA.

Response No. 18: We will review the suggested database and select a benchmark from applicable data.

Comment No. 19: Section 3.13: Add ammonia as a COPEC to MMB Wetland-Surface Water. Chemicals should not be eliminated as COPECs based on background.

Response No. 19: Table 3.13-8 screens Maple Meadow Brook Wetland surface water. Table 3.13-8 indicates that nitrogen (as ammonia) was not selected as a COPEC because the maximum detected concentration was below the screening benchmark, not based on background.

Comment No. 20: Section 3.14. Page 3-29. *CERA* should be changed to *BERA*.

Response No. 20: Text will be revised as noted.

Comment No. 21: Section 3.14. Page 3-29. The ammonia environmental chemistry discussion is very good and helpful; however, in prior meetings between Olin and EPA the discussion included the possibility of trying to identify naturally occurring levels of ammonia associated with wetland sediments (e.g., peat dominated wetlands like those found in the MMB drainage) via a literature search, since the demonstration that observed levels are similar to naturally occurring levels would be a compelling argument for eliminating ammonia as a COPEC. Was this literature search ever attempted? Please provide this information on literature search or justification for chemistry discussion.

Response No. 21: The scientific literature was searched for naturally occurring ammonia concentrations in similar wetland sediment, and found a range so wide that it did not add value to the discussion. Documentation of the search can be provided upon request.

Comment No. 22: Section 3.14: EPA disagrees with the statement on page 3-29 that it is unlikely that ammonia detected in surface water at the site is Site-related because ammonia is a component of the DAPL and other components of the DAPL have been measured in South Ditch. EPA makes the rebuttable presumption that ammonia in South Ditch is Site-related,

which can be rebutted by demonstration that ammonia is not present in groundwater entering South Ditch, by means of piezometers or pore water measurement.

Response No. 22: The conceptual site model, presented in previous reports, indicates that concentrations of ammonia in the South Ditch are impacted by DAPL constituents in groundwater. Text will be revised to clarify that the discussion in Section 3.14 does not apply to South Ditch.

Comment No. 23: Table 3.8-1 is confusing; it might be helpful if the lines in the **Receptors** box for both the Terrestrial Exposure Areas and Aquatic Exposure Areas were removed.

Response No. 23: Table 3.8-1 will be revised to be less confusing.

Comment No. 24: Table 3.9-1: The BCMOE (2010) reference does not appear to be listed in Section 6.0.

Response No. 24: The source will be referenced as:

Science Advisory Board for Contaminated Sites in British Columbia (SABCS). 2010. Guidance for a Weight of Evidence Approach in Conducting Detailed Ecological Risk Assessments (DERA) in British Columbia. Submitted to BC Ministry of the Environment, October 2010.

Comment No. 25: Table 3.9-1. First Column. Typographical error in column heading. Should be “of” instead of “off.”

Response No. 25: Text will be revised as noted.

Comment No. 26: Table 3.9-1. Strength of Association/Description of Attribute notes “Site-specificity and relevance of LOE to assessment endpoint…” According to Menzie et al., the strength of association deals only with the measurement endpoint as it relates to the assessment endpoint and does not consider site-specific information. Site specificity is accounted for in the second attribute “Sensitivity and Specificity.” It is possible that the BCMOE (2010) document noted above makes a good argument for considering it in the “Strength of Association” also, but without seeing that reference, please remove the site-specificity from “Strength of Association” in Table 3.9-1 and cascade the change throughout the report.

Response No. 26: The table will be revised by striking “specificity” from the first column of the second row, as the information in the remainder of the row applies to LOE sensitivity. “Site-specificity” is already discussed in the first row where it has already been identified as an attribute of “strength of association.”

Comment No. 27: Table 3.9-2. Typographical error in column heading. Should be “Rationale” instead of “Rational.”

Response No. 27: Text will be revised at noted.

Comment No. 28: Table 3.9-2. It appears as though a 5-point scale is being used to determine an "inference weight." Either in text or in a footnote, please explain to what categories ranges of average LOE Ranks will be assigned. For example, Average LOE Rank 1 to <2 = "Low."

Response No. 28: A footnote will be added to explain the rankings.

Comment No. 29: Table 3.9-2. Screening benchmark comparisons for the robin and shrew along with food chain modeling are not really independent or separate lines of evidence. Please eliminate the former as there is generally greater uncertainty associated with the benchmark comparisons than with the food-chain modeling.

Response No. 29: USEPA and other regulatory stakeholders often prefer to see the benchmark comparison. Nevertheless, the benchmark comparison will be removed to minimize uncertainty.

Comment No. 30: Table 3.9-2. It seems inconsistent that for Measurement Endpoints1A, 2A, 3A, and 4A, an Average LOE Rank of 2.5 has an inference weight of "Low;" whereas for Measurement Endpoint 3B an Average LOE Rank of 4 has an inference weight of "Medium/High." The suggested explanation of categories noted above may clarify this.

Response No. 30: A footnote will be added to explain the rankings, as noted above.

Comment No. 31: Table 3.9-2. Footnote [a] indicates that the strength of association score is counted twice to double-weight this attribute to account for its importance. It appears that the Average LOE Rank was obtained by dividing the Scores by 5. Please divide the Scores by 6, else values are being moved out of a 5-point scale and do not make intuitive sense. See following hypothetical example:

Attribute	Score	
	w/o doubling	With doubling
Strength of Association	4	8
Sensitivity and Specificity	5	5
Data Quality and Study Design	5	5
Representativeness	5	5
Correlation/Causation/Consistency	4	4
Average LOE Rank by dividing by 5	4.6	5.4
Average LOE Rank by dividing by 6	NA	4.5

In this example, one would expect that double-counting the Strength of Association Score would decrease the Average LOE Rank as its Score is lower than that for three of the other four Attributes. However, dividing by 5 makes the Rank higher.

Response No. 31: The calculation will be revised to divide by six.

Comment No. 32: Table 3.9-2. Although there can be disagreement regarding the actual Score/Rationale given to an Attribute, it is important that the Rationale column give justification for the score using the "Decision Rules" on Table 3.9-1. For example, "There is a high level of

uncertainty associated with the use of benchmarks" is often noted as the rationale for a "Low" Score. Although the low score or the statement itself may not be in dispute, the Rationale does not provide the necessary justification.

Response No. 32: Additional discussion will be provided.

Comment No. 33: Table 3.9-2. Strength of Association. Rationale. "Site-specific conditions…" should not be considered for this attribute (see Comment 22).

Response No. 33: Text will be revised as noted above in the response to Comment No. 22.

Comment No. 34: Table 3.9-2. Data Quality and Study Design. Rationale. "Data used represents numerous years of sampling" is not a rationale for a high score for this attribute. Emphasis should be placed on the fact that sample results used in this BERA followed an approach procedures reviewed and approved by EPA and other stakeholders.

Response No. 34: Text will be revised to emphasize that the sample results used in the BERA followed an approach reviewed and approved by USEPA and other stakeholders. The revision will also emphasize the large number of samples used to characterize site conditions.

Comment No. 35: Table 3.9-2. Correlation/Causation/Consistency. Food chain modeling pages. "Use of TRVs based on specific classes of receptors allows for ability to correlate effects with receptor-specific exposures." This may be true, but there are other issues to consider for this attribute. How well does the observed effect in the study used as the TRV basis correlate with population-level effects? How closely related is the species used in the TRV derivation with the site-specific receptors? Etc. The rationale presented does not match the Decision Rules on Table 3.9-1. (See Comment 28).

Response No. 35: The decision rules are generic, and some interpretation is required to apply them to specific measurement endpoints. The attribute score of 4 for correlation, causation and consistency for food chain modeling measurement endpoints corresponds to the following decision rule:

"LOE response is quantitatively correlated with magnitude of exposure, but correlation is not statistically significant (or data are insufficient to test for statistical relationships); mechanistic linkage inferred, but not definitive."

This decision rule was interpreted to apply to food chain modeling endpoints. TRVs allow for a quantitative measure of effects associated with the magnitude of exposure (*i.e*. a receptors' modeled chemical daily dose), but cannot be tested for statistical relationships. TRVs are typically not available for the specific receptor species, so linkage is inferred, there is some underlying uncertainty. Therefore, the decision rules were followed.

Comment No. 36. Table 3.9-2. Measurement Endpoint 7B. Scores/Rationale appears to be cut and pasted from food chain modeling and are not appropriate for the sediment toxicity test endpoint.

Response No. 36: The text will be revised as noted.

SECTION 4.0 SPECIFIC COMMENTS

Comment No. 37: Section 4.0. Page 4-1. Given the limited reference dataset for surface water and sediment, special care needs to be given when trying to interpret results based on comparisons to reference data.

Response No. 37: This comment applies only to East Ditch (considered separately) and Maple Meadow Brook. Care will be given when interpreting results based on the one reference point approved by USEPA for each water body.

Comment No. 38: Section 4.0. Page 4-1. First set of bullets. Please explain why sediment toxicity testing conducted in South Ditch is not provided as a line of evidence in this section.

Response No. 38: A discussion of toxicity test results conducted in South Ditch will be added to the text.

Comment No. 39: Section 4.1. Page 4-1. RME exposure point calculation using ProUCL should only be attempted with datasets of 8 samples and greater than 50% detected values. Data sets with more than 8-10 samples but <50% detects need special consideration.

Response No. 39: The ProUCL output will be reviewed to confirm that the appropriate statistic was used. Additional discussion will be added to address the uncertainty associated with use of a 95% UCL as an EPC when there are fewer than 50% detects.

Comment No. 40: Section 4.2.1. Page 4-2. 1st Paragraph. Statement “Effects benchmarks represent concentrations at or above which adverse effects are likely to occur.” This is not necessarily true. Effects-based benchmarks are generally LOAELs – doses/concentrations at which effects are noted. The value at which the effect is first seen would lie somewhere between the NOAEL and LOAEL value.

Response No. 40: The statement quoted is directly from MacDonald *et al*, 2000. The nuances between “noted” versus “seen” are unclear in the comment as presented, and any difference are unlikely to affect the outcome of the BERA. No further response is required.

Comment No. 41: Surface Water Effects Benchmarks. Page 4-4. Acute values are not appropriate to use as the only non-screening surface water benchmarks. Acute values may be used to describe site conditions, but they cannot be the only regulations evaluated. Federal criteria and generally state guidelines indicate both chronic and acute values must not be exceeded for particular time periods. This may be addressed in the interpretation of risks but it is not indicated within Section 4.0.

Response No. 41: Acute AWQC are appropriate for use as effects benchmarks in ecological risk assessment because they are based on LC50 and other acute toxicity test endpoints. No changes to the text will be made.

Comment No. 42: Section 4.1 Tables and Attachment 2: There is a 95% ProUCL output provided for Surface Water – Landfill Brook EPC selection (Section 4.0 of Attachment 2), but no associated results table provided in the Section 4.1 Tables.

Response No. 42: Maximum detected concentrations were used to select Landfill Brook COPECs. The ProUCL output was not used to select COPECs in Landfill Brook. The ProUCL output was provided in error. The exposure assessment for Landfill Brook will be removed from the risk assessment because the nature and extent evaluation has concluded that Landfill Brook is impacted by the Woburn Landfill and other industrial properties nearby and not by the OCSS.

Comment No. 43: Table 4.2-1: The Eco-SSL-Mammals for hexavalent chromium is 132 mg/kg rather than 81 mg/kg. Please revise.

Response No. 43: Text will be revised as noted.

Comment No. 44: Tables 4.3-1 through 4.3-15. It is not appropriate to compare plant or invertebrate benchmarks with EPCs. These receptors are sessile or relatively sessile and are not exposed to an average concentration. Sample by sample comparisons are more appropriate.

Response No. 44: Though plants and some invertebrates are sessile, the BERA evaluates risks at the population level, not the individual level. Populations are exposed to concentrations across the spatial extent of the exposure area and over time, conditions best estimated with EPCs based on average concentrations.

Comment No. 45: Tables 4.3-1 through 4.3-15. It is not appropriate to compare acute water quality values with EPCs. Acute benchmarks are maximum concentrations with a not-to-exceed timeframe, which is less than one day.

Response No. 45: Acute AWQC are appropriate for use in ecological risk assessment because they are based on LC50 and other acute toxicity test endpoints. No changes to the text will be made.

ATTACHMENT 5.0 SPECIFIC COMMENTS:

Comment No. 46: Attachment 5. Section 2.0. Page 1. Semi-aquatic wildlife receptors do not include the raccoon, which is listed as measurement endpoint 10B (p 3-17) and on Tables 3.8-1 and 3.9-2. Please include the raccoon food chain modeling estimations.

Response No. 46: The raccoon food chain model will be added.

Comment No. 47: Attachment 5. Section 3.0. Page 2. Equation missing a division sign.

Response No. 47: The text will be revised as noted.

Comment No. 48: Attachment 5. Section 3.0. Page 3. Presentation regarding BAFs is not clear. The hierarchy for selecting literature-based BAFs is not presented. In addition, the bullet list that is meant to describe how BAFs were formulated, when not found in the literature, includes sources for literature-based BAFs (e.g., Baes et al. 1984).

Response No. 48: The text will be revised to clarify how BAFs were derived and selected.

Comment No. 49: Attachment 5. Section 3.0. Page 3. First bullet. Please provide a reference for assuming organic compounds with a Log Kow <3.0 do not significantly bioaccumulate. This type of parameter is generally based on the dataset for a particular study and can vary. For example, the Eco SSL guidance indicates the earthworm regression dataset includes chemicals with Log Kow from 2 to 8. Using both methodologies in the risk assessment is inconsistent. Selection of one methodology over another should be discussed.

Response No. 49: USEPA guidance (USPEA, 2000) states:

"Chemicals with a Log Kow greater than 3.5 are considered to be bioaccumulative, that is, they are likely to partition into organic material, including lipids of organisms and predicted and measured BAFs are correlated within the range of Log Kow 3.5 to 6.5." A lower bound of 3.0 was used to provide a small measure of conservatism. Chemicals with a Log Kow less than 3.0 are not bioaccumulative, even if USEPA included them in the Eco-SSL.

Reference: USEPA. 2000. Bioaccumulation Testing and Interpretation for the Purpose of Sediment Quality Assessment: Status and Needs. Offices of Water and Solid Waste. EPA-823-R-00-001. February.

Comment No. 50: Attachment 5. Section 3.0. Page 3. Second bullet. EPA does not agree with always using Bv values from Baes. Different receptors consume different portions of plants. If the receptor is consuming a reproductive portion (i.e., roots, fruits, seeds), then the Br is a more appropriate value to use.

Response No. 50: Dietary information for receptors is generally not specific enough to justify the use of Br values over Bv values. This uncertainty will be addressed in the text.

Comment No. 51: Attachment 5. Section 3.0. Page 3. Last bullet. Although it may be appropriate to use the same BAFs to estimate terrestrial and aquatic invertebrate tissue concentrations, it is not appropriate to use terrestrial invertebrate soil to tissue BAFs to estimate bioaccumulation from sediment into amphibians or fish. Because the BSAF databases are limited, water concentrations are generally used to estimate tissue concentrations in these receptors. BAFs for such can be found in *Screening Level Ecological Risk Assessment Protocol for* Hazardous *Waste Combustion Facilities* (EPA, 1999) or using EPA's EPISUITE.

Response No. 51: Fish- and amphibian-specific BSAFs were preferentially selected from the literature and were used, when available (approximately 75% of COPECs). BSAFs for

aquatic invertebrates were used as surrogates for fish and amphibians only where BSAFs for fish and amphibians were unavailable. BSAFs for aquatic invertebrates used as surrogates for fish and amphibians were based on soil water to invertebrate tissue equilibrium partitioning models obtained from Jager (1998). Equilibrium partitioning models account for the thermodynamic partitioning of organic chemicals between soil solids, porewater and resident organisms tissues. Jager (1998) uses equilibrium partitioning models to estimate the bioconcentration of organic chemicals from soil water to invertebrate tissue. The partitioning of organic chemicals between soil and soil water in terrestrial systems was assumed to be roughly analogous to partitioning between sediment and porewater in aquatic systems. The Combustion Facilities guidance will be consulted in the preparation of the draft BERA.

Comment No. 52: Attachment 5. Section 3.3. Page 4. 3rd bullet. It is not appropriate to use mammal toxicity data as surrogate data for birds. The differences between the taxa are too great.

Response No. 52: Mammal TRVs were extrapolated to birds (when bird values were not available) based on historical precedent in USEPA Region I, so that there would be a number to evaluate. The BERA will be revised to exclude these extrapolations.

Comment No. 53: Attachment 5. Section 3.3. Page 4. 4th bullet. It is not recommended to use inhalation data to extrapolate oral toxicity to ecological receptors.

Response No. 53: Inhalation exposures were included only where oral toxicity values were not available so as to provide a number to evaluate. The BERA will be revised to exclude TRVs extrapolated from inhalation studies.

Comment No. 54: Attachment 5-1. Dietary intake missing Dosebird.

Response No. 54: The Dosebird value will be shown.

Comment No. 55: Attachments 5-1 and 5-2. Exposure frequency is generally not used in ecological risk assessments. Most toxicity values are based on chronic exposures <1 year or during critical time periods (e.g., reproduction) and would be comparable to the exposure time on-site. If these time periods do not overlap, then the receptor being evaluated is likely not the most appropriate. Any exposure frequency issues are better left to the uncertainty discussion and not dealt with in a quantitative fashion. In addition, if the EF were to be used, the units within these tables do not agree; the definition indicates days/year and the units indicate unitless. The value would have to be a fraction (unitless) for the dimensional analysis to work. Attachments 5-3 through 5-9 indicate units as days/year, so please justify.

1. ***Response No. 55***: The exposure frequency term will be removed from the food chain models. However, Olin would like to reserve the right review the underlying studies and discuss uncertainties of not using an exposure frequency term if the models are showing risk for an analyte when the model is using a TRV based on a longer-term study.

Comment No. 56: Attachments 5-1 and 5-2. Please indicate whether the IR values are in wet or dry weight.

Response No. 56: Ingestion rate (IR) values are in units of grams wet weight per day (g ww/day) as presented in the exposure parameters tables in Attachment 5-3 through 5-9. IR values were obtained from Nagy (2001).

Comment No. 57: Attachment 5-1. Footnote [d]. Assuming the chemical concentration of small mammal and small bird prey is based on ingesting 50% soil invertebrates and 50% plant tissue does not account for the soil ingestion exposure pathway for the prey.

Response No. 57: Body burdens for small mammal and bird prey in carnivore food chain models (*e.g.* hawk, fox) were calculated using a combination of published, peer reviewed, and widely accepted sources available in the scientific literature, including:

- Travis & Arms (1988) to predict uptake body burdens from organic COPECs (*i.e.* B_b) ;
- Baes *et al.* (1984) to predict inorganic body burdens from inorganic COPECs (*i.e.*, F_f); and
- Sample, Beauchamp, Efroymsom, & Suter (1998) to predict inorganic body burdens.

The source used for a given COPEC depends on several factors including medium, receptor, and chemical. Sample *et al.* provides soil-to-tissue uptake factors and so would include incidental soil ingestion of prey species. It is recognized that values from Travis & Arms and Baes *et al*. were developed for agricultural applications for feed ingestion and do not account for soil intake so may underestimate tissue body burdens. Therefore, this under-estimation will be discussed in the uncertainty section. This underestimation is expected to be very minor, because for carnivores, other variables such as site foraging frequency, ingestion rate, and incidental soil ingestion are far more influential to the overall dose calculations than the incidental soil ingestion of prey species.

Olin is not aware of a comprehensive, published, peer reviewed model that includes soil ingestion as a parameter for estimating prey body burdens for all COPECs.

Comment No. 58: Attachment 5-1. Footnotes for water units (e.g., mg/L and L/day) not appropriate to table.

Response No. 58: Text will be revised as noted.

Comment No. 59: Attachment 5-2. Dietary intake missing Doseamphibian.

Response No. 59: Text will be revised as noted.

Comment No. 60: Attachment 5-2. Dosewater has the wrong units and should be in mg/kg-day.

Response No. 60: Text will be revised as noted.

Comment No. 61: Attachments 5-3 through 5-9. Only one site foraging frequency (SFF) is presented. For many of the receptors the SFF will vary per exposure area.

Response No. 61: Calculated SFF values vary by receptor and exposure area. All SFF values are presented in Attachment 5-15. Footnotes will be added to Attachments 5-3 through 5-9 to direct the reader to Attachment 5-15.

Comment No. 62: Attachments 5-3 through 5-9. Exposure duration is listed as a parameter instead of exposure frequency.

Response No. 62: The terminology will be standardized so that all attachments reference exposure frequency.

Comment No. 63: Attachments 5-3 through 5-9. Many of the food ingestion equations are not presented properly as the "b" parameter should be a power (i.e., superscripted).

Response No. 63: Text will be revised as noted.

Comment No. 64: Attachments 5-3 through 5-9. Soil ingestion rates appear to be calculated based on wet weight ingestion rates mostly using data from Beyer et al. This is incorrect as Beyer et al. based their % on dry weight ingestion rates and soil concentrations are in dry weight. Please correct.

Response No. 64: Soil ingestion rates calculated per Beyes *et al.* (1994) will be based on dry weight food ingestion rate. The following example is provided to show how the values from Beyes will be used. These equations will be shown in the revised food chain modeling Attachment 5 to be provided in the next deliverable.

Example 3. Calculating the soil ingestion rate for the American robin.

Beyes reports that robins ingest soil at a rate 10% of the daily food ingestion rate, on a dry weight basis:

$$IR_{soil} = 10\% * IR_{food} \qquad \text{(Equation 4)}$$

Where:

IR_{soil} = Soil ingestion rate (kg/day dry-weight)
IR_{food} = daily food ingestion rate (kg/day dry-weight)

For the robin, the food ingestion rate was calculated using a regression equation for insectivorous birds (Nagy, 2001), which provides a food ingestion rate in terms of wet weight:

$$IR_{food} = 1.633 * BW^{0.705} \qquad \text{(Equation 5)}$$

Where:

IR_{food} = daily food ingestion rate (g/day wet-weight)
BW = body weight (g).

Substituting a value of 82 g for the weight of the robin (Attachment 5-3),

IR_{food} = 37 g/day wet-weight = 0.037 kg/day wet-weight.

Next, IR_{food} must be converted from wet weight to dry weight. To do so, the dietary composition and water content of each dietary item must be considered:

IR_{food} (dry weight) = IR_{food} (wet weight) * P_{invert}*(1-WC_{invert}) + IR_{food}*P_{veg}*(1-WC_{veg}) (Equation 6)

Where:

IR_{food} = daily food ingestion rate (kg/day dry-weight)
IR_{food} = daily food ingestion rate (kg/day wet-weight)
P_{invert} = Proportion of invertebrates in diet (unitless)
WC_{invert} = water content of invertebrates (unitless)
P_{veg}= Proportion of vegetation in diet (unitless)
WC_{veg} = water content of vegetation (unitless)

Substituting:

- dietary composition values provided in Attachment 5-3:
  P_{invert} = 0.85 (85% invertebrates)
  P_{veg} = 0.15 (15% vegetation)

- water content values from USEPA (2005) "*Attachment 4-1: Guidance for Developing Ecological Soil Screening Levels (Eco-SSLs) Exposure Factors and Bioaccumulation Models for the Derivation of Wildlife Eco-SSLs*":
  WC_{invert} = 0.85 (85% water)
  WC_{veg} = 0.84 (85% water)

- and IR_{food} = 0.037 kg/day wet-weight (from Equation 5),

IR_{food} = 0.0058 kg/day dry-weight.

Finally, substituting the IR_{food} (dry-weight) value of 0.0058 into equation 4 yields the dry weight soil ingestion rate:

IR_{soil} = 0.00058 kg/day dry-weight.

Table 1 (attached) summarizes dietary composition, corresponding water content, receptor body weight, equations to calculate food ingestion rates (wet weight), and calculated soil ingestion rates for all food chain model receptors considered in the BERA. Table 1 also provides justification and sources for those values.

Comment No. 65: Attachment 5-3. Dietary composition text indicates that the robin consumes only invertebrates yet the dietary fractions in the model included 32% vegetation. Please fix inconsistency.

Response No. 65: The text will be revised so that the text and the model agree.

Comment No. 66: Attachment 5-3. The 10 % soil ingestion rate seems reasonable, please reference the woodcock value provided in Beyer et al 1994.

Response No. 66: The *Beyer et al.* (1994) reference for woodcock will be provided as a surrogate for the robin.

Comment No. 67: Attachments 5-4 and 5-6. Site foraging frequency. If a receptor is obtaining only 1,000th of its diet from the Site, it is not an appropriate receptor to be evaluating.

Response No. 67: Receptors were selected to cover the full range of trophic classes of organisms that may use site exposure areas. Higher tropic level animals such as the red-tailed hawk and red fox require large home ranges for hunting prey. Although a larger home range limits the exposure of higher tropic level organisms to site, risks to these organisms are still relevant to the ecological risk assessment and to future risk management activities.

Comment No. 68: Attachment 5-4. Home range (red-tailed hawk). Appears to have been calculated using data from the four different seasons; however, the exposure duration indicates most adult pairs in New England migrate south during October/November and returning in February/March. If this is the case, then the home range data should not account for times the animal is not there. This would effectively cut the home range in half.

Response No. 68: "Exposure Duration (or Frequency)" is the parameter which accounts for time spent in New England. As shown in Attachment 5-4, the food chain model assumes that red-tailed hawks in New England begin the southern migration in mid-October and return in mid-March, spending approximately 210 days per year (approximately 7 months) around the site.

"Home Range" describes the extent of the nesting and foraging territory. The home range value was calculated as the average of values published for breeding pairs of red-tailed hawk in USEPA's Wildlife Exposure Factors Handbook. As home range is independent of exposure duration, it is not necessary to divide the calculated home range value to account for migration.

Comment No. 69: Attachment 5-3. Please provide some rationale beyond "assumption" for the hawk soil ingestion rate.

Response No. 69: A reference will be provided.

Comment No. 70: Attachment 5-4. Food ingestion rate for the shrew is based on a regression equation for insectivorous mammals; however, the dietary compositions used in the modeling are 14% plant and 9% mammal. The omnivore equation may be more appropriate.

Response No. 70: Since invertebrates were assumed to comprise the vast majority of the shrew's diet (77%), the insectivorous mammal equation is more appropriate than the omnivore equation.

Comment No. 71: Attachment 5-4. Beyer et al., 1993 is cited as the reference yet the footnotes indicate Beyer et al., 1994. The 1993 document is the pre-publication copy. Please reconcile.

Response No. 71: The reference will be revised to Beyer *et al*. (1994).

Comment No. 72: Attachment 5-7. Site foraging frequency value is zero. Please correct.

Response No. 72: SFFs vary by exposure area and are presented in Attachment 5-15. A footnote will be added to Attachment 5-7 to direct the reader to Attachment 5-15.

Comment No. 73: Attachment 5-7. Food ingestion rate for the marsh wren is based on a regression equation for omnivorous birds; however, the dietary composition used in the modeling is 100% invertebrates. Please reconcile.

Response No. 73: Revising the food ingestion rate for the marsh wren from the equation for omnivorous birds to insectivorous birds would change the ingestion rate from 0.0092 kg/day to 0.0086 kg/day. After accounting for rounding, both are essentially equal at 0.009 kg/day. There is essentially no difference between the two equations, and changing the equation would result in a negligible change to risk calculations. The food chain model will continue to use the incumbent value.

Comment No. 74: Attachment 5-7. EPA recommends the using a soil ingestion rate of 10% for the marsh wren based on information provided in EPA's *Screening Level Ecological Risk Assessment Protocol for Hazardous Waste Combustion Facilities, Volumes 1, 2 & 3. Peer Review Draft*. (EPA530-D-99-001A)

Response No. 74: The food chain model assumes a 9% soil ingestion rate, as cited in Beyer *et al.* (1994). USEPA has accepted this source for other receptors. The food chain model will continue to use the incumbent value.

Comment No. 75: Attachment 5-7. Body weight. The actual value looks to be in the correct range; however, the notes indicate that the value is the average adult value for studies done in freshwater habitats when it appears to be the average for GA salt marsh dwellers.

Response No. 75: The values are for studies done in GA salt marshes the reference notes will be revised accordingly.

Comment No. 76: Attachment 5-8. Home range. Please use real data and not assumptions without any rationale for the calculation of the green heron's home range.

Response No. 76: The reference for home range will be provided in the draft BERA

Comment No. 77: Attachment 5-8. Food Ingestion Rate. The omnivorous bird calculation use seems inappropriate given the all animal-based diet. In addition, EPA references indicate ingestion rates of approximately 0.243 kg/day, which is substantially higher than the calculated 0.06 kg/day.

Response No. 77: Olin will review the ingestion rate for the green heron. The source of the 0.243 kg/day value is not provided in EPAs comment, and therefore the value cannot be evaluated or confirmed at this time.

Comment No. 78: Attachment 5-8. EPA recommends using the mallard soil ingestion rate of 3.3% as provided in Beyer et al 1994, as a substitute value for the green heron.

Response No. 78: The green heron incidental soil ingestion rate will be revised from 9% to 3.3%..

Comment No. 79: Attachment 5-9. Please include a reference and/or notes for the dietary composition percentages. Values not clearly from EPA, 1993. Please reconcile.

Response No. 79: Additional notes will be provided regarding the selection of dietary composition percentages for the muskrat.

Comment No. 80: Attachment 5-9. Food ingestion rate. Please indicate which equation is being presented.

Response No. 80: Additional notes will identify which equation is used.

Comment No. 81: Attachment 5-9. EPA recommends using a soil ingestion rate of 2.2% for the muskrat based on information provided in EPA's *Screening Level Ecological Risk Assessment Protocol for Hazardous Waste Combustion Facilities, Volumes 1, 2 & 3. Peer Review Draft*. (EPA 530-D-99-001A). .

Response No. 81: The muskrat incidental soil ingestion rate will be revised from 9.8% to 2.2%.

Comment No. 82: Attachment 5-9. Inhalation rate and surface area rows should be removed.

Response No. 82: These rows will be removed. Inhalation rate and surface area were not used in the food chain modeling equations.

Comment No. 83: Attachment 5-10. The chemical list is in no apparent order and it is difficult to locate individual chemicals. Please alphabetize in some fashion.

Response No. 83: The chemical list was arranged by CAS Number. The table will be revised to a more "user-friendly" format.

Comment No. 84: Attachment 5-10. Please provide missing references for values (e.g., terrestrial invertebrate values for thallium and antimony).

Response No. 84: The missing values will be provided.

Comment No. 85: Attachment 5-10. Please explain why there is no terrestrial plant BAF for mercury.

Response No. 85: The scientific literature was reviewed but an appropriate value could not be located.

Comment No. 86: Attachment 5-10. Small mammal, small bird, and fish/amphibian BAFs could not be verified as the footnotes do not provide sufficient information to recreate values.

Response No. 86: Footnotes will be revised to provide additional information.

Comment No. 87: Attachment 5-10. Notes. 3rd line. Something appears to be missing. Incomplete thought.

Response No. 87: Text will be revised.

Comment No. 88: Attachment 5-10. Notes. [b].2. It appears that plant BAFs for organics were calculated using the Travis and Arms, 1988 equation. More updated methods are available for estimating plant concentrations. The Eco-SSL derivation documents should be referred to for guidance; for example the Eco-SSL guidance has regression equations specifically for estimating plant concentrations of PAHs.

Response No. 88: Olin will review the Eco-SSL guidance and revise as appropriate.

Comment No. 89: Attachment 5-10. Notes. [b].2. Please provide a reference for assuming forage is 80% water.

Response No. 89: Footnotes will be revised to provide additional information.

Comment No. 90: Attachment 5-10. Notes. [b].3. Please split this footnote into two to elucidate when Sample et al., 1988 versus Baes et al., 1984 is used for the BAF derivation.

Response No. 90: The footnote will be revised.

Comment No. 91: Attachment 5-10. Notes. [b].3. Because of the unclear footnoting, it was not possible to easily check where this may be an issue; however, it should be noted that if "preference was given to recommended regression estimates," the Bechtel Jacobs regression estimates result in concentrations in plants and not plant BAFs. Concentrations would be calculated on an exposure area-specific basis and no BAF would be presented.

Response No. 91: The footnote will be revised to clarify the method used.

Comment No. 92: Attachment 5-10. Notes. [c].2. and [e].2. Please clarify these footnotes. It is not possible to calculate the terrestrial/aquatic invertebrate BAFs based on the information given herein. In addition, footnote [c].1/[e].1. where it is indicated that bioaccumulation is

assumed to be 0 for organic analytes with log Kow <3.0 appears to be in conflict with that in [c].2. that indicates that BSAF are not estimated for analytes with log Kow <1.

Response No. 92: The footnotes will be clarified.

Comment No. 93: Attachment 5-10. Notes. [c].3/[e].3/[e].4. Terrestrial/aquatic invertebrate BAFs could not be verified. As for plants, please note that if Sample et al., 1998 regression equations are used, the result is concentration in soil invertebrates, not invertebrate BAFs. Concentrations would be calculated on an exposure area-specific basis and no BAF would be presented.

Response No. 93: The footnotes will be revised to clarify the method used.

Comment No. 94: Attachment 5-1. Note [d].3. Refers to Sample et al., 1998 for BAFs for small mammal and birds. The reference for Sample et al. included in the table is only for soil to plants.

Response No. 94: The footnote will be clarified.

Comment No. 95: Attachment 5-10. Notes. [e].3. Does not seem to make sense. Please rephrase.

Response No. 95: The footnotes will be clarified.

Comment No. 96: Attachment 5-10. References. Please correct. Sample et al, 1998 is noted as “Empirical Models for the Uptake of Inorganic Chemicals from Soil to plants… BJC/OR-133.” The reference of this title is a Bechtel Jacobs publication. Sample, B. E., J. J. Beauchamp, R. Efroymson, G. W. Suter II, and T. L. Ashwood. 1998 may refer to two different documents: 1) Development and validation of bioaccumulation models for earthworms. ES/ER/TM-220. U.S. Department of Energy; or 2) Development and validation of bioaccumulation models for small mammals. ES/ER/TM-219. U. S. Department of Energy.

Response No. 96: The footnote will be clarified.

Comment No. 97: Attachments 5-11 through 5-14. Because of the issues noted below for Attachments 5- 11 through 5-14, it was not attempted to verify individual TRVs.

Response No. 97: No response required.

Comment No. 98: Attachments 5-11 through 5-14. Please explain why different TRVs were selected for small versus large receptors and what the differences in the selection processes were between them.

Response No. 98: Text will be revised to provide the rationale.

Comment No. 99: Attachment 5-12. The marsh wren is not a large bird. Toxicity values for such are not appropriate. Olin should use the small bird TRVs presented in Table 5-11 for assessing risk to the marsh wren.

Response No. 99: Olin will review the TRVs in Table 5-1.

Comment No. 100: Attachment 5-13. The muskrat should probably not be considered a small mammal. It weighs >100 times that of the "small" short-tailed shrew and less than 4 times that of the "large" fox.

Response No. 100: Large mammal TRVs will be used for the muskrat.

Comment No. 101: Attachment 5-15. Exposure area in hectares for EA-2 and EA-5 are not consistent with those presented on pages 3-5 and 3-8, respectively.

Response No. 101: Text will be revised so that exposure areas are consistent.

Comment No. 102: Attachment 5-15. Calculations of American robin and red fox SFFs for EA-5 are incorrect.

Response No. 102: SFFs will be reviewed and corrected if necessary.

Comment No. 103: Attachment 5-15. Exposure media indicates "no" for invertebrates for the red fox whereas Attachment 5-6 indicates soil invertebrates are 5% of the diet.

Response No. 103: Text will be revised so that "exposure media" are consistent.

Comment No. 104: Attachment 5-15. How the exposure areas for semi-aquatic species are calculated needs further consideration. Species such as the marsh wren forage in wetlands and calculating the exposure area based on the waterbody itself would not be correct. Species such as the green heron and muskrat home ranges are not equivalent to waterbody area. A home range is based on how far an animal will travel and oftentimes semi-aquatic species need to have the shoreline foraging length considered. Neither is equivalent to the size of the waterbody. Data to estimate more accurate SFFs may not be available; however, uncertainty discussions should be added to account for the lack of data and the potential effect on the risk estimate.

Response No. 104: Olin will evaluate how exposure areas are calculated based on EPA's comments and will provide a discussion of the associated uncertainties.

Comment No. 105: Attachment 5-15. This table should include exposure area information for the terrestrial and MMB background locations.

Response No. 105: The requested information will be provided.

Comment No. 106: Calculated Exposure Point Concentration Tables. For the most part, the calculations in these tables were not checked because of the issues noted regarding life history exposure parameters and BAFs noted above. However, it was noted that some estimations appear to be incorrect. For example on Table EA-4 RME Red-Tailed Hawk – 1, Concentrations calculated for Cbird and Cmammal are different even though the soil concentrations and BAFs

are the same. In addition, neither of the concentrations appear correct for many of the chemicals.

Response No. 106: Values presented in the tables will be reviewed.

EAST DITCH SCREENING LEVEL ECOLOGICAL RISK ASSESSMENT

Comment No. 107: List of Figures, TOC ii: There is no Figure 1.0-1 provided in report.

Response No. 107: The Site Locus Figure 1.0-1 will be provided in the report.

Comment No. 108: Section 1.0, Page 1-1. Change "The East Ditch is a portion OU2" to "The East Ditch is a portion of OU2".

Response No. 108: The text will be revised.

Comment No. 109: Section 1.0, Page 1-1. Change "in order refine" to "in order to refine".

Response No. 109: The text will be revised.

Comment No. 110: Section 3.2, Page 3-1: Change "Phase II Focused Environmental Risk Characterization Addendum East Ditch and Downstream Areas, 51 Eames Street, Wilmington Massachusetts, RTN 3-0471 (MACTEC, 2004)." To Phase II Focused Environmental Risk Characterization Addendum East Ditch and Downstream Areas, 51 Eames Street, Wilmington Massachusetts, RTN 3-0471 (MACTEC, 2004), hereafter referred to as Focused ERC.

Response No. 110: The text will be revised.

Comment No. 111: Change "The 2013 environmental characterization included" to "The 2013 Focused ERC included".

Response No. 111: The text will be revised.

Comment No. 112: Section 3.2.1, Page 3-2: Change the sentence from: "The absence of deposited sediment forming islands or point bars throughout the ditch resulted in an optimal sediment deposition score." to: "The absence of deposited sediment forming islands or point bars throughout the ditch resulted in a marginal or poor sediment deposition score." Since the sediment score was always poor or marginal.

Response No. 112: The text will be revised.

Comment No. 113: Table 3.2-1 Habitat Assessment Summary. Sediment deposition should be poor for: north of Eames St. overpass, South of Eames St. overpass, and 1,000 ft south of Eames St. overpass and marginal for: 200 ft south of south ditch outfall and 500 ft north of Anderson Station.

Response No. 113: The text will be revised.

Comment No. 114: Attachment 2 and Section 3.3, Page 3-4: MassDEP endangered species review provides a satellite image of the area but please be more specific about what is being shown on the satellite image from the MassDEP TES results even if there are no endangered species.

Response No. 114: The purpose of Section 3.3 and Attachment 2 is to document the that protected species have not historically been observed and no priority habitats have been mapped. Additional discussion of what is shown on the satellite image is beyond this intended purpose of the discussion.

Comment No.115: Attachment 3-3: no page numbers.

Response No. 115: Page numbers will be added to Attachment 3-3.

Comment No. 116: Attachment 3-4: no page numbers.

Response No. 116: Page Number will be added to Attachment 3-4.

Comment No. 117: Table 4.1-2: Check calculated values – the wrong numbers are being reported.

For example (if using a hardness value of 106):

Copper CCC=0.013 mg/L should be changed to 0.015 mg/L (or 14.8 µg/L).

Copper CMC=0.016 mg/L should be changed to 0.0098 mg/L (or 9.8 µg/L).

Also, equations should account for conversions from µg/L to mg/L. Equations for dissolved metals should be provided in addition to the equation for total metals; e.g., CMC=exp{mA [ln(hardness)]+bA}*Freshwater Conversion Factor.

Response No. 117: Olin will review the calculations and make any necessary revisions. Olin will also provide the equations for dissolved metals showing how the conversion factor is applied and will show unit conversions.

Comment No. 118: For COPEC selection, the minimum and not the average hardness should be used to calculate hardness-based criteria as this will provide more conservative criteria values. Sample-specific hardness can be used to further refine the risk estimate.

Response No. 118: Using average hardness to calculate hardness-based criteria provides values that are appropriate to assess the prevailing conditions within a given exposure area. Furthermore, risk estimate refinements and the risk characterization assess the average condition over time and across the full spatial extent of the exposure area and at the population level. It is therefore not necessary to characterize risk on a point by point basis.

Comment No. 119: The new benchmark values calculated should then be carried through the rest of the COPEC selection and refinement of COPECs.

Response No. 119: See response to Comment No. 118.

Comment No. 120: Table 4.1-2: In the Notes section, [a] change "expressed at mg/L" to "expressed as mg/L"

Response No. 120: The footnote will be revised.

Comment No. 121: Section 4.3.1, Page 4-3 and Table 4.3-1: Acetone is listed as a COPEC but should be deleted based on results in Table 4.3-1.

Response No. 121: The text will be revised. Acetone is not a COPEC in East Ditch surface water.

Comment No. 122: Section 5.3, Page 5-4. Acetone should be deleted from this surface water COPEC list, as it was eliminated based on Table 4.3-1 in the 1st COPEC screening level effects evaluation.

Response No. 122: The text will be revised. Acetone is not a COPEC in East Ditch surface water.

Comment No. 123: Section 5.3: It is stated that there is no benchmark for iron in surface water; however, there is a water quality criterion for iron (1000 ug/L). Please revise as appropriate.

Response No. 123: Text will be revised.

Comment No. 124: Section 5.3, Page 5-3. Change "…iron or bromide therefore" to "…iron or bromide and therefore". Change "…respectively), therefore iron and bromide in can…" to "…respectively), and therefore iron and bromide can…".

Response No. 124: Text will be revised.

Comment No. 125: Section 5.3, Page 5-4: Chloroethane should be added as a COPEC for surface water in the Effects level screening refinement of COPECs. It is included in section 6.1.

Response No. 125: Text will be revised to include chloroethane.

Comment No. 126: Section 5.4, Page 5-4 Sediment COPEC refinement and Table 5.4-1: It is not appropriate to eliminate Chromium and Nickel from the list of sediment COPECs given that the site sediment HQs are only being compared to one sediment reference HQ.

Response No. 126: The East Ditch and reference sediment datasets are limited for several reasons. First, very little sediment has deposited in the railroad ballast that forms the bottom and banks of East Ditch limiting the amount of sediment available to sample. Second, USEPA agreed to only one reference location. One sample may not rise to a strict definition of "reference", but it does provide some local context. Considering that chromium and nickel in East Ditch sediment are only marginally higher than their respective effects benchmarks (average HQs are 1.1 and 1.0, respectively), and considering the very poor habitat quality of the East Ditch, adverse population level effects to ecological receptors

from concentrations of chromium and nickel in the East Ditch are unlikely to occur. The one reference sample provides an additional line of evidence that chromium and nickel concentrations are consistent with off-Site conditions. Olin will provide this discussion in the risk characterization of the SLERA.

Comment No. 127: Section 6.1.2: The chemical n-nitrosoid-n-propylamine is mentioned in several places. Please confirm the name of this chemical.

Response No. 127: The text will be revised to state the correct name of the chemical as n-nitrosodi-n-propylamine.

Comment No. 128: Section 6.1.3: Please provide the reference for Uniroyal (1992) in the response to these comments. There is additional aquatic toxicity information for Kempore which does not change the conclusions of the SLERA. There is now a completed dossier for Kempore (C, C'-azodi(formamide, CAS no. 123-77-3) in REACH (http://echa.europa.eu/web/guest/information-on-chemicals/registered-substances). The Probable No Effect Concentration is 0.289 mg/L which is lower than the average concentration of Kempore in surface water (1.0 mg/L); however, the NOEC for fathead minnows is 50 mg/L, the solubility limit, and the NOEC for 21-day Daphnia magna reproduction is 2.89 mg/L (affects on reproduction occurred at 9.19 mg/L). Based on the approach used in the SLERA, the refinement step would reach the same conclusion as is presented in this section. Please add this information and the Uniroyal (1992) reference if the SLERA is going to be revised.

Response No. 128: The reference for Uniroyal (1992) is:

Uniroyal (1992) Unpublished report from the Uniroyal Chemical Company Inc., Middlebury, CN. As cited in: World Health Organization (WHO), 1999. Concise International Chemical Assessment Document 16: Azodicarbonamide. World Health Organization. Geneva, 1999. Available at:

http://www.inchem.org/documents/cicads/cicads/cicad16.htm#SectionNumber:6.1

Olin will add this citation and will review the information from the REACH dossier for Kempore.

Comment No. 129: Section 6.2.1, Page 6-3: Change "ecological receptors from theses COPECs" to "ecological receptors from these COPECs".

Response No. 129: Text will be revised.

Comment No. 130: Section 6.2.1, Page 6-3: Change "man-made channel runs parallel" to man-made channel that runs parallel". Change "East Ditch is convey" to "East Ditch is to convey".

Response No. 130: Text will be revised.

Comment No. 131: Section 6.2.3, Page 6-3: "Although HQs for metals in East Ditch sediment were > 1, average HQs from metals in East Ditch were all <1, indicating that exposure of benthic and amphibian populations across the entire exposure area and over time is unlikely to result in adverse effects." The first half of the sentence is true but the second half of the sentence should be altered to be more specific about the individual samples rather than lumping all 13 metals across all of the samples.

Response No. 131: HQs based on average concentrations were used to characterize effects that occur over time and across the full spatial extent of a given exposure area, and at the population level. It is therefore not necessary to alter the second half of the sentence to consider risk on a point by point basis. The average condition is a sufficient basis upon which to characterize population level risk.

Comment No. 132: Section 6.2.5, Page 6-4: Delete "is unlikely" at the end of the sentence. Last sentence: change "Risk are likely" to "Risk is likely".

Response No. 132: Text will be revised.

Comment No. 133: Please see comment on BERA report on OU1 and OU2 Section 3.12.1 regarding selection criteria for choosing an LC50 from ECOSAR and possibility of consideration of receptors other than fish. The same comment/question applies to this East Ditch SLERA for surface water.

Response No. 133: See response to comment 17.

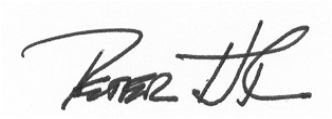
Comment No. 134: Attachment 4: Footnote [f] of the ECOSAR Surface Water Screening Benchmarks Calculations indicates that aquatic effects benchmarks are calculated by applying a multiple of 10 to calculated screening benchmarks. Please confirm that this procedure is provided in the TNRCC,2000b methodology and that the benchmarks represent a loweffect benchmark, rather than a no-effect benchmark.

Response No. 134: The methodology is consistent with the TNRCC guidance. The effects benchmarks are low effect benchmarks (LOAELs). Equation 2 shows the wrong operator and should correctly appear as: NOAEL = LC50 / f. Equation 2 will be revised to show the NOAEL divided by the conversion factor (f), not multiplied currently shown. The same correction applies to the corresponding equation in the BERA (Equation 1).

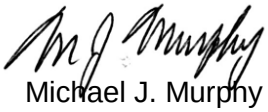
If you have any questions concerning this letter, please feel free to contact James Cashwell at (423) 336-4012.

Sincerely,

AMEC Environment & Infrastructure, Inc.



Peter H. Thompson
Project Manager



Michael J. Murphy
Project Principal

cc: James Cashwell, Olin
Joe Coyne, MassDEP
AMEC Project File

Project File \\PLD2-FS1\Project\Projects\olinwilm\Olin Wilmington CSS 2014\4.0_Deliverables\4.1_Reports\OU1 OU2 RI\RTC Letters\2014 03 21 Wilmington - OU1 OU2 BERA_No.2_RTC.docx